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The Lazarus Manifesto for Positive Psychology and Psychology in General

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AUTHOR'S RESPONSE

The Lazarus Manifesto for Positive Psychology and Psychology in General

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The great value of journals like *Psychological Inquiry* lies in the prospect of an honest and informed exchange of ideas, which could flush out the highly individual ways that we, as psychologists, look at the same issues and, one hopes, clarify diverse positions. As long as we don't get personal and only talk past each other, such exchanges permit us to appreciate how great the void is that often separates the ways we think about all sorts of issues.

Except for the first section that follows, which is about the debate itself, my strategy in this response to the commentators is to center most of my attention on broad substantive issues. There are three major themes that underlie my view that advocates of positive psychology have misdirected our field. Everything I say in this author's response is closely connected to these three themes.

Theme 1 is that the arguments about positive psychology reflect two broad views of life. The first view is to look at life as it actually is, with its inevitable stresses, struggles, and crises. A positive outlook on life depends on the coping process, which can integrate good and bad, positive and negative, and often transcend the negative.

The second view, which is that of positive psychology, makes a false dichotomy out of the positive and negative rather than integrating them. It opposes, avoids, minimizes, or denies the realities, though this too is denied. It idealizes the search for a never-ending happy experience of life. However, it masks this outlook within a set of human virtues, which, at first blush, are difficult to second-guess because of social correctness; everyone wants to be on the side of virtue. In short, this movement presents an almost Pollyanna version of the Garden of Eden notion of the good life and good people while it masquerades as being tough-minded and scientific.

It is fine to be a good and competent person, so there is no argument here, but we never really learn what it means to be good and competent. I think positive psy-

chologists are constantly congratulating themselves for having discovered positive character traits all by themselves. This typology of positive and negative, which the positive psychology movement seems to be stuck with, is unproductive because the realities of life usually fall in between and, to be even more precise, most people seek to integrate the extremes of positive and negative, thereby making the best of the negative and often creating positive out of negative.

There is an irreconcilable difference between my own view of life and what appears to be the outlook of the positive psychology movement. The approach I favor is more intraindividual—that is, it is found within a given person rather than being based on a contrast of positive and negative types of people. Mine is not solipsistic, however, because the good life cannot be found without recognizing the negative side of life and seeking relationships with the world as it is and other people while accepting them as they are. The outlook that people seek a middle ground within which to view life is epitomized, by the way, in the two seemingly contradictory frames of reference that I say people use in negotiating an appraisal. *Negotiation* is where the integration of opposites comes in.

I have said in the target article and elsewhere (e.g., Lazarus, 1999) that most people try to view what is happening realistically in order to cope effectively but at the same time try to give this reality a positive spin in order to preserve sanguinity and hope. Unfortunately, how the negotiation process works in appraising, which is really the most important issue, has not yet been studied to my knowledge. It is, as I said, to be found within a single person who tries to even the negative realities with something positive to believe in and live by. It is not static or merely a matter of virtuous traits but depends on an ongoing process of negotiation.

Everyone must deal with threatening realities and favorable possibilities. The contradiction between them is resolved in some way by this negotiation. This principle of negotiation becomes the basis of my claim that posi-

tive and negative are inseparable and the resolution depends on coping. Throughout my author's response this set of ideas keeps coming back in different ways. Some of the key terms for my way of thinking are *intraindividual*, *coping*, *negotiation*, and the *interdependence* of positive and negative.

Theme 2 is that the idealized Garden of Eden version of the positive psychology movement lacks conceptual clarity. There is, for example, a failure to examine what is and makes for a good life and what positive and negative actually mean. Is positive, for example, defined by optimism, mental health, success, happy feelings, favorable social relationships, service to others and the community, a sterling character, and so forth? Until positive psychology clarifies all this and more, it can never make coherent sense.

Theme 3 consists of the methodological problems that are not being faced by the positive psychology movement or, for that matter, by psychology in general. One problem is the scarcity of longitudinal or intraindividual perspectives in research, which makes it difficult to infer causation and to describe an individual fully over time and circumstance. If this problem is not acknowledged and dealt with, the positive psychology movement will be unable to obtain adequate empirical support for what it wants to claim or to make good sense in light of the nature of life.

The research of this movement fails to tell us what is happening in the minds, actions, and social relationships of individual people, a problem that applies to psychology as a whole—namely, the tendency to overgeneralize about people. This leads protagonists of the movement to overlook the ubiquity of individual differences in just about everything they think, feel, and do and to end up with oversimple statements that actually apply to no one.

These broad themes undergird most of the specific issues that I deal with. Until these themes are confronted, clarified, and dealt with, I see little chance that the positive psychology movement is going anywhere. These themes dwarf in importance the nitpicking that is necessary in the next section about the so-called debate that never really comes off between yea-sayers and naysayers, but the debate seems to do better in the hands of like-minded supporters of my arguments as they try to sharpen the issues. To overcome the failure of the two opposing sides to come to terms with each other, I have tried throughout to identify the important issues.

The main topics I take up in my author's response, all of which grow out of my target article and the commentaries, are verbalized roughly in a number of main section headings in this article. They include the debate itself; health, successful aging, and measurement; individual differences revisited; whether our research methods are capable of doing justice to our speculations about the mind and adaptation; causality

and description in science; when the science underlying interventions is faulty; whether psychology is overly focused on stress, struggle, and crisis; coping and emotion; and finally, how the worrisome problems I have been discussing came about.

About the Debate Itself

I have divided the commentaries into those that were largely critical of what I wrote and those that were supportive. I take up most of the first group early in this section; I have saved the supportive ones for later to emphasize points about coping and emotion and to provide me with an uplift. I respond to two others, Ryff (this issue) in a separate section dealing with health, successful aging, and measurement and Deiner (this issue) and Ryff (this issue) in another section on individual differences. This pattern allows me to conclude the unpleasant task of defending my position early on so I can proceed thereafter on a more constructive note.

One strategy that the most critical commentators used was to rebut what I never said and ridicule implications I never drew, which has the effect of obfuscating the issues and distorting what I said. A second is to attack the least important of my criticisms and ignore the truly important ones, especially the methodological ones, which might have embarrassed many researchers. A third strategy is to fill their replies with glowing, unrealistic restatements of the positive psychology outlook.

After reading some of the commentaries, especially those of Rand and Snyder (this issue), Seligman and Pawelski (this issue), Csikszentmihalyi (this issue), and Ryff (this issue), I had the feeling that I must have disturbed a hornet's nest and the hornets had come out to sting the enemy invader. My critics succeeded, however, in instantiating the existence of an ideological movement of like-minded persons who are inclined toward a very narrow mind-set and resent any criticisms, whether or not they have merit. What these protagonists have done is to transform what I said into grounds for attack. Most often its senior spokesperson offered only very general, ambiguous statements without significant substance.

I have tried not to respond in kind. However, as a person who does not readily turn the other cheek, I probably reveal my distaste for their stridency by being testier here than in the target article. I suppose it is like a lawyer in a trial who can claim more liberty in cross-examination when the witness is shown to be hostile. Fortunately, some commentators are highly supportive. Even when others are highly critical of what I wrote, a few of them offer comments that contained real substance.

In this first main section, About the Debate Itself, there are four subtopics: Distortions of My Views, The

Thin Brew of Positive Psychology, Religion and Science, and So What Else Is New?

Distortions of My Views

I should first point out that I did not actually say, as many commentators allege, that positive psychology is a fad that lacks future potential. What I did say was that the movement is in danger of becoming a fad, like many others before it, if it doesn't shape up. I have, however, all but abandoned my earlier reservations about making a premature forecast that this movement was going nowhere. The closed mindedness and zealotry exhibited by its progenitors and their associates convinced me it was. They just "didn't seem to get it."

In their surprisingly snide commentary, Rand and Snyder (this issue) misrepresent what I said and seek only to reaffirm an idealized characterization of positive psychology. Nothing is said, for example, about methodology, though it was the most detailed feature of my criticisms. Their commentary is also a paeon to Seligman, who has done creditable work in the past on learned helplessness and explanatory style. We are evidently expected to engage in hero worship for his efforts on behalf of positive psychology—his version, that is.

Their commentary makes it all but impossible to respond appropriately without seeming to be silly and on the defensive. They say, for example, that I launched "an obloquy at a person or group of people associated with positive psychology," (Rand & Snyder, this issue) and that this was presented with ire. Where in my target article have I denigrated anyone? Where is there evidence of ire? I would say it is nonexistent. All I can do is to affirm once and for all that what these protagonists say is inaccurate, to say the least, and disappointing if anyone hoped for a reasoned debate from them.

Seligman and Pawelski's (this issue) commentary is no more constructive. It is so out of touch with what I said in my target article that I ought to ignore it, but, of course, I cannot do that in a journal that has the purpose of stimulating debate. Nevertheless, it makes no sense to waste too much of this essay by opposing each of their assertions one by one, which would end with boring repetitions of "they said this, but I said that," hardly an edifying contribution. Therefore, I have been selective in my rebuttal and prefer to go beyond what I had said in the target article. Their commentary is also a disappointment.

These commentators dismiss my methodological challenges with a wave of the hand, as it were, saying "the bulk of the literature in the fields of *optimism* [italics mine] and of flow consists of longitudinal studies" (Seligman & Pawelski, this issue). This is not accurate. When I looked at Nakamura and Csikszentmihalyi's (2002) chapter on the concept of flow in the *Handbook of Positive Psychology*, I found brief mention of three longitudinal studies and a number of others with meth-

odological characteristics that I could not identify with confidence. They may well have been cross-sectional.

It could be true that many studies of flow are longitudinal—flow is a temporal phenomenon—and to his credit, Csikszentmihalyi uses innovative monitoring procedures to assess daily experiences. However, the literature on optimism, like most psychosocial research, has consisted largely of cross-sectional studies of college students based on questionnaires. Seligman, who conducted some of the better longitudinal work in this arena, is well aware that this is so.¹

The Thin Brew of Positive Psychology

In the target article I complained that to promote their ideology, many proponents of positive psychology ignored much of the work of the past, not just mine but that of a large number of other distinguished research scholars. Seligman and Pawelski (this issue) now want to substitute the phrase *psychology as usual* for the earlier phrase *negative psychology*, though I am not sure why, and I do not see how it helps their cause. Everyone already knows what is really meant—that the psychology of the past was fixated on stress, struggle, and crisis rather than the most positive emotional qualities and desirable lifestyles of which people are capable.

However, as is discussed later in this author's response, I reject the notion that traditional psychology has accentuated the negative. I believe we, as psychologists, have actually underplayed it, and I try to make that case later. Much good usually comes from the struggle to transcend the recurrent and sometimes chronic harshness that is inevitable in human life. However, the correct view for me is not a dichotomous typology but an integration of both positions.

In place of the original list of constructive personal qualities that I quoted in the target article from Seligman and Csikszentmihalyi's (2000) preamble for positive psychology, Seligman and Pawelski (this issue) now offer a new set of virtues—namely, "valor, kindness, originality, fairness, and future-mindedness." They intimate that these traits, like the earlier list, make for only good outcomes, such as socially desirable lives, success, satisfaction or happiness, and flow.

There is a great deal of flimflam in this, which should not be overlooked. Serious questions remain to be addressed about this list or any list like it. For example, how are these presumably desirable traits defined—that is, what do the words naming them really mean? What are the biological and social origins of these traits? Under what conditions do they manifest themselves? How do

¹To document this, a search of PsycINFO between January 2001 and August 2002 revealed that 86 publications included the word *optimism* in the title. Deleting chapters, dissertations, and reviews from the list leaves 53 journal articles on optimism of which only 11 (20%) were longitudinal.

they work? None of this is obvious, and there is no attempt in their commentary or other writings to provide the necessary answers.

Positive psychologists plead for time. They should by all means take as much time as possible in order to provide the world with a more thoughtful and complete account of their outlook than is currently on the table, one that might also be founded on better science. I am not clear what the rush was to get into print with mundane ideas that are so poorly thought through.

There is another equally important question that must be addressed if what Seligman and Pawelski (this issue) are proposing is to make sense. We should, for example, ask them what the conceptual and empirical grounds are for implying that only a good outcome can flow from these traits. Is it not possible, even likely, that under certain conditions, and in certain kinds of persons, these presumably wholesome person qualities could also provoke unhappiness or even misery? In this connection, I recently heard about (but have not yet seen) a book being edited by Chang and Sanna (in press) that addresses the potential consequences of so-called virtues. In any case, Seligman and Pawelski are offering a thin brew that has little intellectual depth, and this seems typical of too much of the writings of the protagonists of positive psychology.

Religion and Science

I am also disturbed by something else in Seligman and Pawelski's (this issue) commentary that is tricky to pin down and discuss. In my opinion, they are promoting a kind of religion, a vision from on high, which is falsely clothed in a claim to science that never materializes. There is nothing inappropriate about seeking functional spiritual meanings and studying qualities that might contribute to a socially valuable and satisfying life. Nevertheless, it is the readiness to make populist-sounding pronouncements, which are offered without clarity about what these traits really are, that troubles me. The so-called desirable qualities are presented without empirical foundation or a thoughtful rationale. They are slogans rather than well-worn truths. What they say goes beyond the pale of scientific discussion.

This could be expected from a minister or guru rather than from a scientist. As I read what they say, I found myself thinking I must have entered a house of worship rather than a scholarly or scientific debate. As with even well-organized traditional religions, a different set of tenets and rules for living from another guru is certain to be rejected. If you hold opposing views, you cannot get to heaven, or maybe worse, you will fail to obtain a research grant.

If, as psychologists, we are doing science, we should be able—no, we must be required—to examine whether what we are proposing actually applies and under what conditions. If we do not entertain and examine unher-

alded alternative possibilities, we have abandoned science. More and more, positive psychology seems populist and intellectually much too easy rather than a set of thoughtful ideas or principles to be respected.

What I just said also applies to the Templeton Foundation, which Seligman and Pawelski (this issue) mention approvingly. It too promotes religion. This is, of course, their own business, but the foundation also supports research on positive psychology themes and provides prizes, which makes it our business, too, because its agenda could bias the choice of research topics and even what is found.

The philosophers whose writings the foundation cites approvingly assert that religion can be dealt with under the rubric of science, but I am very doubtful of this. Science cannot support or refute religious ideas because they are matters of faith. It is not so much that Seligman and Pawelski (this issue) make pronouncements about what is good or bad that troubles me. All sorts of people, including me, do this all the time. It is the vagueness, the religious tone, and the arrogance with which they are made that gets under my skin. The protagonists of positive psychology do not appear willing to consider the possibility that they could ever be wrong.

So What Else Is New?

About my assertion that there is nothing new in the positive psychology movement, Csikszentmihalyi (this issue) asks rhetorically "Who ever claimed it was [new]?" Yet on an earlier page of his commentary he had written "I know full well that new ideas can be killed just as soon by uncritical acceptance as by opposition." I think he protests too much, as do other club members defending the flag.

My assertion seems to have been a sore point for a number of commentators, so let me say a bit more about newness. I too am a Johnny-come-lately, and so are we all, to refer in passing to the comment by Rand and Snyder (this issue). Why should people get all riled up about whether what they wrote is new unless they were determined to claim newness? Being exercised by the idea that what one has said is not new is likely also to be historically naive. Few of us say anything that is truly new, and just about everyone must come to terms with this because most modern ideas have counterparts in the historical past.

Psychologists have the habit of coining new words to express old ideas and using the same old words to express what they promote as new. This has long been a major source of confusion. Yet we spend little time examining the meanings of ambiguous terms that are in constant use. It is also impossible to read everything these days, even in our own specialties, so we have become increasingly ahistorical.

In his commentary, Diener (this issue), who was generous in speaking of my work, called me both a cur-

mudgeon and a positive psychologist, which gave me a hearty laugh. I can readily accept both of these characterizations with comfort as long as he doesn't call me an optimist. What I said about negotiation in appraising makes me only one half of a positive psychologist—that is, the half that deals with positive spin.

As a number of commentators noted, my criticisms do, indeed, apply to psychology in general and not just to positive psychology, and I said this right off in the target article. It led quite a few commentators to suggest that this should let positive psychology off the hook, as if it was anathema to challenge the movement when the same problems can be found in psychology as a whole. I still fail to grasp why the assertion that much of psychology is similarly wrong-headed should protect positive psychology as long as the criticisms are valid. It does not imply a double standard—that is, that positive psychology must do better than psychology at large, as Ryff (this issue) suggests. On the contrary, its logic is merely that both positive psychology and psychology in general need to improve.

My thoughts that something was wrong with the positive psychology movement actually came into being before I realized that its most serious defects—consisting especially of simplistic, black-and-white thinking and largely inadequate methodology—also applied to much of psychology. The sudden prominence of the positive psychology movement initially provoked me to try to think through what was bothering me, which led to an American Psychological Association talk and, subsequently, this target article. Both were to some degree fueled by the gung-ho, self-congratulatory hype of the protagonists of the movement, which seemed to suggest that they had discovered something about which those who had come before had not been aware. The hype continues unabated.

I end this section with a few further reactions to two other commentators, King (this issue) and Lyubormirsky and Abbe (this issue). King's commentary pointed to the need for balance between positive and negative, but I think, as is already evident, that the emphasis should actually be even more in the negative direction than it currently is. I take this up later in a full major section.

In any event, her commentary (King, this issue) made me wonder whether the story she tells about her seat neighbor on the plane communicated as much about the misunderstandings that psychologists have about people who are troubled as the way troubled people try to understand what is wrong. Given the traditional Western value system, which is to take responsibility for one's woes rather than blaming others or fate, why should it be surprising that individuals often look within themselves for clues about where they have gone wrong? I don't see what, if anything, her story has to do with the imbalance of emphasis that positive psychologists complain about.

King also suggests that the subject matter of positive psychology is far broader in scope than I acknowledged, though I never claimed it was narrow. On the contrary, it is so broad and diffuse that it is difficult to characterize, except for the positive outlook, which is the element that brings the positive psychologists under the same tent. One of my messages in the target article was that the problem of that movement is not so much what it studies but the way the studies are designed. She keeps insisting that research has shown this or that, to which I react that given the typical methodology, it may well not have been shown at all.

Lyubormirsky and Abbe (this issue) quickly seem to dispense with the happyology theme. They then assert that realists are more likely to be chronically unhappy and suffer adverse outcomes, so we are back again to happyology after all. By the way, there is no muddier concept in the history of at least 2,000 years of philosophy and psychology than the nature of happiness (see, e.g., Strack, Argyle, & Schwarz, 1991; Veenhoven, 1990). I also question whether being consistently happy should be regarded as essential or perhaps even relevant to the good life.

These authors actually claim many things—for example, that there were many fruits of positive psychology, that stable increases in happiness are possible, and that there was strong evidence of the benefits of happiness. I reserve judgment because more than in any other commentary, these authors support what they say with a long list of studies without any information about methods or any reasons for conviction or doubt that a good case had been made.

They go on to say that whether pessimism, realism, or optimism is more adaptive can be resolved only with well-controlled empirical investigations. Amen! They accuse me of ignoring mounting evidence of the advantages of optimism, but they then proceed to ignore opposing evidence. It is difficult to know what to say about all this except that in this commentary it is evident that we have not yet escaped the methodological and interpretive doldrums created by much of the positive psychology movement. So much for most of the hornets—I am pleased to move on from this part of my task to more enterprising issues.

Health, Successful Aging, and the Issue of Measurement

Ryff has made major contributions to health and successful aging, which are relevant to the concerns of positive psychology and my target article. In her commentary (this issue), she is critical of my statement that more careful and in-depth measurement is needed in the study of stress, emotions, and coping. Here I comment first on her ideas about health and successful aging and then take up her criticism about measurement.

I am most familiar with the target article Ryff published with Singer (Ryff & Singer, 1998) on health as wellness, concepts I view as closely related to the arena of coping. Ryff and Singer have been among the most recent to argue for an enlarged concept of wellness and against the traditional medical concept of health as the absence of disease. Echoing earlier writers, these authors have taken the position that positive health is more than the mere absence of illness, and they speak of it in the broadest sense of the word, which includes both physical and mental. This sounds good at first, but the criteria of wellness remain vague.

I see a major danger, however, in the proposition that mental and physical health should be combined into a single concept of wellness, especially in the context of aging. If they are tied together too closely, they could do a disservice to the elderly or even a young person who is ailing or physically disabled, by overlooking the substantial psychological integrity and effort required for transcending serious physical ailments and living successfully in spite of them.

Health or wellness then becomes an ideal that is seldom to be found, certainly less so among the elderly who are declining physically but are substantially in good shape psychologically, even taking into account some loss of stamina and working memory. It might be better to keep the physical and psychological separated despite some degree of interdependence between them. The interdependence arises because a person who is older or sick needs a certain amount of energy and physical strength to function well, particularly when he or she must compensate for physical deficits and limitations (see Baltes & Baltes, 1999, with respect to the concept of compensation).

Ryff and Singer (1998) went on to make a point that is consistent with what I have just written, and I have written it many times before. Difficulties and traumas of living can facilitate finding deeper life meanings (see also Stroebe, Hansson, Stroebe, & Schut, 2001, for much more about this). In other words, people often extract positive psychological growth from negative life experiences, as in grieving over a major loss, which commonly requires considerable psychological struggle. What I just said should not be viewed as an endorsement of positive psychology. It simply recognizes that positive can come from negative, not by magic but as a result of determination to cope with stress and adversity as well as one can. Why psychological growth takes place in some persons and not others is a complex question that is not easily answered, but it falls within the broad rubric of coping.

What does wellness of mind mean? It could refer, among other things, to a sense of well-being, contentment (or mood), the capacity for enjoyable experience, reality-based cognitive functioning, or effective coping. The same problem applies to conceptions of the good life. It is difficult to speak clearly and consis-

tently about this without more definitional clarity. However, whatever it is said to be, I think that wellness of mind must be viewed in a relativistic way—that is, it should be based on how well people do psychologically given their physical limitations. There can be no uniform or universal standard of successful aging. It depends on what we as human beings have to compensate for psychologically and physically and the resources we have available to do so.

These resources include environmental supports provided by society, which are often crucial for how well people can function when they get older and are ailing. At a minimum, it should reflect whether and how well they can manage to live independently (see, e.g., Salthouse, 1997). This society, incidentally, does the least along these lines of any so-called advanced country in the world (see, e.g., Crimmins & Hayward, 1997). If what people have to deal with in life is daunting in relation to their resources, less should be expected from them. Yet, one must also acknowledge that persons who are older and many of the young who are disabled often greatly exceed expectations in this regard, which should give them high marks for resilience, courage, determination, will, or whatever one wants to call it.

I found a great deal of good sense in Ryff and Singer's (1998) criteria for successful aging. These criteria include three kinds of engagement in living: first, living purposefully, which centers on having meaningful goals, objectives, and pursuits; second, maintaining close ties with others; and third, having positive self-regard and mastery. To these, however, I would add the coping processes that support the effort to compensate for deficits and limitations, thereby allowing the person to function.

In emphasizing coping, which seems to be of little interest to positive psychologists, I am suggesting that the concepts of positive psychology, by themselves, are insufficient for the study of successful aging. There must be the will to sustain one's life values and goals even against the odds. Where the will to do this comes from is obscure. Successful aging is likely to require a determined effort to cope with limitations, deficits, and loss. Three of the commentators, Tennen and Affleck (this issue), Folkman and Moskowitz (this issue), and Campos (this issue), concern themselves with coping, so I come back to it again later in the main section on coping and emotion.

About the need for careful, in-depth measurement (description) of stress, emotions, and the coping process, Ryff (this issue) writes disparagingly in her commentary, saying that "the essence of this [Lazarus's] complaint appears to lack substance." I think she is totally wrong. Rand and Snyder (this issue) are also highly critical of what I said about measurement as it applies to hope, perhaps because Snyder approaches hope more as a trait than a process.

Whether hope and anxiety—one should read this more generally as whether any two emotions—can occur together simultaneously is interesting and important. Ryff (this issue) flatly asserts that they cannot and proposes that these mental states must be sequenced over time. I don't know how she knows this, but I respond with two points, one of them in the form of a rhetorical question and the other a statement giving the reason I consider her wrong.

My question is as follows. Why can't we hope while waiting for a biopsy report and at the same time experience anxiety that it might be cancer? Both are often present together in the same person–environment encounter. Are we unable to juggle two emotions at the same time? Is this a well-established finding?

As to my assertion, the very basic question itself belies her dismissal of my recommendation that the experience of emotions in any given encounter must be assessed with great care. The right question to ask concerns what it means to speak of emotions as occurring together. Although we do not know the truth, there are at least three possibilities. The first two are that (a) more than one emotion can be experienced at the very same instant and (b) two emotions can occur within an extended encounter that is defined by a common interpersonal business but at different moments as a result of a fluctuating focus of attention, which seems to be similar to the temporal sequencing that Ryff (this issue) seems to favor.

Incidentally, my colleagues and I raised a similar issue about why many different coping processes are reported as having been used by participants in any single stressful encounter (see Folkman, Lazarus, Dunkel-Schetter, DeLongis, & Gruen, 1986; Lazarus & Folkman, 1987). We also thought this could be due to sequencing and also offered a rationale—in effect, a person may try one approach but sees that this does not work and, therefore, changes to another. The third possibility, (c), is that maybe all three alternatives could apply, depending on the circumstances.

The fact that the answer is not known means that the issue being disputed requires measurement. To provide a viable answer, it would be necessary to follow the subjective emotional experience, the coping processes—this includes both thoughts and actions—in an encounter, as closely as possible over time. In effect, my point about the necessity of careful, in-depth measurement has plenty of substance and, for that matter, importance too. I don't understand why Ryff (this issue) was so dismissive.

Individual Differences Revisited

One of the most important themes of my target article was the neglect of individual differences, the significance of which I detailed in my criticisms of the Gross

et al. (1997) study of emotion and aging. Their findings showed that the overlap between the older and young cohorts was so large that little could be said about how they differed in their emotional lives, though this was presumably a primary purpose of the study.

I want to come back to this issue because what I said was not warmly supported by commentators, even some whose reactions were otherwise generally favorable. Diener does not see the overlap as at all important, which I view as a serious mistake. He comments that the nonoverlap of 25% in the Gross et al. (1997) study was, in his view, more stunning than the 75% overlay. Diener says it points to the many possible causes other than chronological age that are probably operating in each cohort. However, there is no empirical information from this study about such causes, nor could there be, given the cross-sectional research design.

Yet, here is the point I want to make. At this juncture, I am not speaking about causation but about how individual differences limit what can be said about the emotional patterns of people who are older compared with people who are young. In this respect, there is also a defect in Diener's calculations. The 25% he speaks of must be divided in half to consider each separate cohort that sits at the two opposite poles of the distribution—in effect, 12½% for the older cohort and 12½% for the younger one. Thus, we are really speaking about only a little more than one out of eight older people and one out of eight young people, hardly enough to provide a viable portrait of the difference in the emotional lives of older and young people.

The limitations of the research design are, therefore, not just in the matter of a cohort's confounding the attempt to demonstrate causality. They also have to do with individual variations within each cohort, which does not have any more cachet in cross-sectional research than it does in laboratory experiments. What can be said, therefore, about the emotional lives of young and older people from this research when it leaves out most of the participants? Not much.

There are two ways of studying individual differences, one in which individuals are compared with each other, the other in which they are compared with themselves over time or under divergent life conditions. As I said in my target article, the way we ask questions makes a substantial difference in the answers we obtain (see Epstein, 1983, whom I cited there). This brings us back to our old friend, ipsative–normative research designs. Thus, if we ask about the health of older persons, an observer is apt to use others as the norm for such a comparison, but the older people themselves tend to compare themselves with how they were at an earlier time. The former research design is normative, or interindividual; the latter is intraindividual.

If older persons occasionally compare themselves with others, perhaps because they are ego involved in wanting to look vigorous or see themselves as healthy,

they may report accordingly. However, if they compare themselves with the way they were in the past, they will probably give the contradictory answer that they are ailing. Both answers are, in a sense, correct because they are responses to different questions and the only sound solution would be to use both formats. It is by no means trivial whether a given study uses one or the other research designs.

Ryff (this issue) is not much impressed by my allegation that individual differences were largely ignored by psychologists in their research because they were almost always looking for general principles that applied to most people. She notes with apparent amazement that I did not mention the field of personality, which she presumes must be focused on individual differences by definition. It is, but her assertion that personality psychologists do not view individual variation as an embarrassment in the search for generalizations about people is just plain wrong.

Personality psychologists focus on individual differences but not for the purpose of describing richly any individual persons or the actual dynamics of their relationships with others but to make generalization about personality traits. Clinicians make rich descriptions. Great writers do too; we call them *great* because their literary descriptions offer important and intimate truths about mythical people that usually have a concrete basis in the writers' reality.

The great irony is that the field of personality has for a long time been one of the worst offenders in displaying a lack of interest in describing individuals, except, perhaps, superficially rather than in depth. Trenchant criticisms by Block (1995, updated 2001) of the Big Five traits as descriptors of personality make this point effectively.

Many years earlier, Carlson (1971) made a strong case in *Psychological Bulletin* that personality psychology showed little interest in what people are like as individuals but was primarily concerned with generalizing. The title of her article asked rhetorically, "Where Is the Person in Personality Research?" Her analysis received much attention at that time, but the issue, though it has evidently faded from consciousness, has never really gone away, especially if one is clinically oriented. Here too, as on the topic of measurement, Ryff (this issue) is inappropriately dismissive of what I said.

Are Research Methods Capable of Doing Justice to Our Speculations About the Mind and Adaptation?

I want to share a personal concern that may have played a role in my methodological critique in the target article. Almost as long as I have been a research scholar, I have sensed a troubling gap between two seemingly contradictory features of psychology, espe-

cially in those subfields that focus on psychosocial processes. Many of the ideas that psychologists, philosophers, and great writers of literature have formulated about the mind and human adaptation are impressive, but we, as researchers, are able to draw on only the most limited procedures designed to provide empirical support for these ideas. We must somehow transcend this gap between speculation and proof by developing better approaches and putting them into practice. I believe this has always been the most important challenge for psychology.

I find myself surprised to be a methodological whistle-blower, given my ultimate disaffection with the laboratory experiment, which most other academic psychologists, myself included, in the period just after World War II believed was the soundest approach for evaluating our theories and speculations. A large proportion of my research career, over 25 years from the late 1940s to the mid-1970s, has been devoted to laboratory research. In those early days, there was a tendency in academic psychology to overvalue such experiments.

The control offered in the laboratory, which is supposed to be its most precious asset, is apt to be illusory because the interpersonal relationships between experimenter and participant are complex and varied. Even though the experimental procedures are held constant for all participants, these individuals make diverse appraisals of what is happening—for example, concerning their personal stakes in what is happening and the purpose of the study.

I first tried to shift to field research in the 1970s because I finally recognized that procedures used in the laboratory did not provide adequate analogues for the rich and varied processes we needed to understand in the arena of stress, emotion, and coping. After that I consistently advocated the use of in-depth interviewing and multimethod approaches—for example, ipsative–normative formats, single-participant designs, laboratory experiments, or clinical interviews, which commentators Matthews and Zeidner (this issue) also emphasize for studying the ways people behave and react in adapting to life's demands and opportunities.

I appreciate what Matthews and Zeidner (this issue) say about the *grain size problem*, which addresses the largely overlooked issue of the optimal categorical breadth for any variable or process one wishes to study. In discrete emotion theory, the size of a given emotion category—for example, whether anxiety and fear should be a single category or treated as two related but separate categories or whether different kinds of anger (e.g., righteous and pouting) should be separated because their psychodynamics differ—remains a major issue. Their commentary is replete with interesting and clear ideas; it also includes a discussion of the weaknesses of another popular concept that could turn out to be a fad, emotional intelligence.

I have also been an advocate of multiple measures and have often used them within the same study to reflect different levels of analysis—namely, observable actions, subjective reports, and physiological changes (Lazarus, 1998). Obtaining all one's data from a single method or level of analysis risks creating the problem of method variance in which the relationships one finds are obtainable only with that method.

My impression has been that even the best research methods available fail to measure up to the lofty reach of our speculations. This makes it seem all the more shameful to me that so often the task of understanding the most complex creature on the face of the earth is approached in such a halfhearted way. Psychologists are much too willing to settle for the weakest available methods. Some of these methods, such as the all-too-popular cross-sectional formats and survey studies using casually developed questionnaires, are adequate for no more than pilot studies. Because replications and follow-up studies are seldom to be seen, these studies often end up being final rather than preliminary.

Nor do I exempt myself from this criticism. As I said in the target article, my colleagues and I (Folkman, Lazarus, Pimley, & Novacek, 1987) performed an often-cited cross-sectional study in an effort to determine the role of age in patterns of stress and coping. It was a costly mistake because we could not adequately answer the central questions we had asked.² Even commentators who seem to agree with me about methodology exhibit what I think is a tendency to soft-pedal or whitewash the inadequacies. This does not, however, apply to Harvey and Pauwels (this issue), Tennen and Affleck (this issue), Matthews and Zeidner (this issue), and Campos (this issue), all of whom expand helpfully on what I had said in the target article.

Causality and Description in Science

Two of the most prominent values of science are causality and description. In the target article I used the term *measurement* rather than *description*. However, full description, regardless of how it is done, is absolutely essential for adequate understanding. In an era in

which science is almost universally defined as the search for causes, too little attention is paid to the actual phenomena of interest.

Description or measurement by itself, however, is not enough. The world is normally portrayed as orderly. If we, as researchers, want to understand it, not only out of curiosity but for the purpose of making things better, causality remains an essential feature of the scientific enterprise. The principle that every event is caused by something else is called *determinism*. The major problem with this idea is that many of us have begun to recognize that strict determinism cannot be applied without qualification. Two complications are involved in this recognition.

First, given the multiplicity and hierarchical complexity of potential causes, we can predict less well than we once thought. Interested readers might examine Somerfield and McCrae (2000) and Lazarus (1998, 2000) for recent discussions of multiple causation, practical applications, and appropriate research designs in the field of stress, emotion, and coping.

Second, the behavior of living creatures—humans especially—is governed by intentions. This does not translate well into the inanimate physical world. Many psychologists take seriously some variant of free will in which human beings need not always act or react automatically as the external and internal causal influences dictate. I am perfectly willing to speak of will, though it is a concept that bothers psychologists who are addicted to a strict determinism.

As human beings, we can, however, overcome addiction, control drinking and smoking, and even destroy ourselves by determined acts of will, which is sometimes referred to as agency, though we never have total control and often have only the illusion of control. An oddity, incidentally, is that when we think this way, the word *determinism* is transformed into the word *determination*, which is a motivational construct that presumably facilitates efforts to pit oneself against powerful but unwanted causal pressures.

These complications result in strange contradictions between the way psychologists think informally about cause and effect in their daily lives and the way they assume it operates in their research, where they act as though everything is caused yet all the while accepting a "soft determinism." Years ago I knew some strict Watsonian behaviorists who, when speaking of family, friends, and colleagues, never hesitated to explain their behavior with reference to intentions and desires. Psychology was defined as the science of behavior in those days, a time when one might have been laughed off an academic podium for even mentioning the mind.

The practical importance of cause-and-effect thinking can be seen clearly in clinical medicine, which is

²Anyone who is interested in a high-quality methodological debate about whether psychosocial interventions can improve clinical outcomes in organic disease might want to read the series of discussions by six contributors—namely, Lundberg (2002), Markowitz (2002), and Williams, Schneiderman, Relman, and Angell (2002). Williams et al. made a pungent statement about meta-analyses that I think also applies generally to studies with good and bad research methodologies. He wrote, "You cannot strengthen your argument... simply by accumulating a lot of studies that are not very good. A large number of weak studies do not add up to a strong conclusion" (pp. 566–567).

intended to help us manage life-and-death issues and the physical and mental losses and deficits that can result from illness and aging. In the practice of clinical medicine, if the conditions that affect how human beings function are known, it is believed that something constructive can be done about illness or its resulting dysfunction. As a practical matter, if a clinician wants to treat an ailing patient, the odds that a drug or surgery will result in a cure have to be high enough to warrant the risks.

In clinical psychology, however, practitioners might try to teach disabled persons to cope more effectively and make it easier for them to live independently, which is of the utmost importance in advanced age. If, however, as psychologists, we choose an inadequate method for our research, we face the danger of doing harm or losing credibility—this is one way in which we can cease to be experts in the eyes of the public.

One might think, by the way, that a concern about doing serious harm would not apply to positive psychology, as if its concepts offered little to worry about because recommendations are not likely to be life threatening. Not true. Consider, for example, the case of unproven alternative medicine programs, the benefits of which, if any, could represent placebo effects because they often depend on psychological mediation rather than direct physiological changes.

Patients can die quickly of certain cancers if they abandon useful medical care for alternative psychological approaches rather than treating the latter as supplements. This abandonment can sometimes be a deliberate or barely conscious but intentional death-inducing decision because the patient rejects the prospect of a slow, debilitating ending to life when it seems to be inevitable.

Consider also pronouncements about how to live life that are made in the absence of adequate empirical support—for example, how to cope with this or that harm or threat or to have a successful old age. They may do serious harm when they are not well-founded or are severely incomplete in that they overlook the contexts in which they are to be applied. My target article and author's response consists of efforts to advance the idea that lack of rigorous thinking and sound methods should not be waved off by lip service. As I said, the harms that can be done are substantial, and they apply also to positive psychology.

When the Science Underlying Interventions Is Faulty

What is needed now is to bring this point about harm home with some concrete examples. For this pur-

pose I turn to some recent instances in clinical medicine of serious harms, which are usually referred to as *iatrogenic disorders*—meaning they were medically caused—and that stem from a poor research base.

A recent well-publicized event can be used to highlight the actual harm that can be done when the empirical basis of medical procedures is inadequate. As I began writing my author's response to the seven commentaries that had already arrived by mid-July 2002, a major public commotion appeared in the media. It was provoked by an unusual decision on the part of the National Institute of Health to cancel a large double-blind study evaluating the effects of hormone replacement therapy for women at menopause.

The preliminary findings of this study had apparently made clear to the federal researchers that the two drugs used in the treatment, estrogen and progesterin, in addition to preventing other ailments and easing the bodily symptoms of menopause, had created too many added health risks. This news created major uncertainties for millions of women about whether they should begin, continue, or cancel the treatment. They had to weigh the following information about risk. Of 10,000 women who took the drugs for a year compared with the same number of control participants who did not, the drug takers would experience 8 more cases of invasive breast cancer, 7 more heart attacks, 8 more strokes, and 18 more blood clots. There would also be 6 fewer colorectal cancers and 5 fewer hip fractures.

At first glance the magnitude of these added risks and even the benefits seem almost trivial. Yet, if these figures are reasonably accurate, the added risks in public health terms translate into almost 25,000 cases of life-threatening, handicapping, and terminal illnesses for the roughly 6,000,000 women using hormone replacement therapy. Some officials were quick to point out that the treatment, which is designed to be preventive, is for a universal life change that is actually not an illness or major dysfunction in itself, though for some women the menopausal symptoms can be extremely troubling.

What is most relevant to my general argument, however, is how this debacle came about and why the health risks were not evident earlier, before use of hormone replacement therapy had become widespread. What seems to have happened is that there was at first some modest theory and a few inadequate small-scale studies. A body of opinion began to emerge that made the procedure seem desirable from a health standpoint despite skimpy evidence. As was pointed out with consummate journalistic skill in a series of articles on the *Boston Globe* op-ed pages of July 15 and 16, 2002 (Moore, 2002a, 2002b), the practice of medicine appeared to have gotten ahead of the science that was supposed to support it. The reasonable suppo-

sitions on which the treatment was predicated had simply not been adequately supported and were later belied by the large federal study that was suddenly terminated.

This is hardly the first time this kind of thing has happened as a result of the failure to do careful research before instituting major treatment programs. One horrifying example is the drug DES, which was given to pregnant women in order to prevent miscarriages. It took many years to discover that the drug did not work. Far worse, some of the daughters of those who took the drug developed vaginal cancer. There are plenty of other examples of the harm that can result from inadequate research. Medicine is an art, not a science, and physicians have too little time to keep careful track of patient outcomes, though they would prefer to rely on medical science as much as possible. Again and again, one sees examples of this problem in which belief, wishful thinking, and inadequate research mislead almost everyone and cause harm.

In keeping with these examples of the consequences of weak research designs, statistics about risk factors for this or that terminal or disabling illness and advice about what to do constantly bombard the public. Who among us can properly apply these statistics to themselves as individuals? They are statements about probabilities for the population as a whole during a particular historical period or in a given geographic location (see also Diener's, this issue, commentary). Thus, if one has a blood cholesterol level that is said to be too high, one is presumably at risk for heart attacks or cerebral strokes. If one smokes, one is said to be at risk for lung cancer, bladder cancer, emphysema, or chronic obstructive pulmonary disease (COPD). Nevertheless, many persons with a high blood cholesterol level or too much of the bad kind of cholesterol have sound cardiovascular systems, and many who have smoked all their lives do not get the ailments associated with this bad habit. Still others get lung cancer, emphysema, or COPD without ever having smoked.

It could be misleading to apply illness probabilities simplistically to particular individuals because these individuals may have unmeasured protections or, perhaps, even greater than average vulnerability to a given illness. This is because our knowledge about causes is much too limited. Because of the uncertain individual outcome for any statistical risk, people must decide for themselves whether or not to smoke or load up on saturated fats. Making this decision is a constant source of concern for large numbers of people, which connects with the next major issue I discuss, which has to do with the first theme with which I began this essay—that is, how we portray the realities of life.

Is Psychology Overly Focused on Stress, Struggle and Crisis?

It will come as no surprise that I liked Harvey and Pauwel's (this issue) commentary about the ironies of positive psychology. I found nothing of importance with which to quarrel. Like me they believe that the movement actually understates the negatives of life—for example, death, trauma, and loss. In saying this, they turn the positive psychology criticism of our field on its head. It is the positive psychology movement that is unbalanced. Harvey and Pauwel are, incidentally, the only commentators to take this position. Their observations about returning to normalcy after trauma and questions about growth are also valuable and not often discussed.

What these commentators write about negative experiences in life made me think of a recent television program about Eleanor and Franklin Roosevelt, which suggested that it took a long time for both to cope successfully with separate and joint adversities. Even though their accomplishments, individually and jointly, are remarkable, they continued to suffer from and struggle with earlier traumas. For example, Franklin came down with paralytic polio at an early peak in his career, and he could never walk or stand independently from then on. Eleanor had to deal with a lack of marital warmth, parental coldness and neglect, and the disparaging idea that she was ugly.

Yet both were loved and admired by much of the public, myself included, and they were both often upbeat, as celebrated in Franklin's smiling face and rakish cigarette holder that always pointed up. Most of us understand that public presence is not always a good guide to private outlook and emotions. Nevertheless, case histories such as this reinforce what I said in the target article about the double-sided nature of positive and negative.

I suggested in the target article that to have a major personality change might require something like a personal crisis. It is apt to be one in which meanings and ways of coping that were once serviceable are no longer adequate to preserve sanguinity or even the minimal ability to function (see Slaikau, 1984, on crisis intervention; see also Lazarus, 1999). However, in their commentary, Tennen and Affleck (this issue) offer what they consider a minor quibble about this tendency to emphasize the rhetoric of struggle or crisis. They suggest that the emphasis on life as a struggle has "emboldened positive psychology," which could leave the impression that those who study stress and coping are somehow responsible for helping to create this movement.

I do not believe that we should accept any degree of blame for positive psychology and its weaknesses. Why should an important truth ever be suppressed or softened even if it might result in some distress? Unimportant truths, which are often referred to as *white lies*,

are commonly dealt with differently because without good cause we see no point in hurting someone we care about. Yes, indeed.

However, it is often a questionable strategy to suppress the truth—for example, in order to make a very ill or dying patient feel better. It might really have the aim of protecting distressed relatives from going overboard. Patients feel pressured to deny the painful truth of their plight and to minimize the public manifestations of their distress. Instead, they need to be loved and reassured that it is okay to complain and cry about their fate. Many years ago I wrote about this dilemma as the *trivialization of distress* (Lazarus, 1985).

I want now to make the point as strongly as I can that we should not back away from a view of life as a struggle for most people some of the time and other people most of the time. There is an old song that implies that effort is needed to manage this struggle with good spirits. One line of it goes, “Look for the silver lining and *try to find* [italics added] the sunny side of life.”

Ordinary life stresses—for example, common disasters, such as automobile accidents, accidents, earthquakes, hurricanes, tornadoes, explosions, floods, and fires—are hardly uncommon. Even if such disasters never happened to us as individuals, they are apt to be present in our minds as potential threats to our existence, and to some extent we build our lives around warding them off—for example, by wearing seat belts, buying cars that are rated as relatively safe, and installing electronic warnings in our homes.

To this must be added many ailments, such as emphysema and other disorders, in which sick older people must struggle hourly and daily simply to breathe. Consider, too, the many young victims of asthma. Another common type of ailment consists of autoimmune diseases in which the muscle strength to keep the eyes open or to walk is insufficient and for which poisonous medicines must be used to make it possible to function at all. Again, this is a small sample of ailments.

One of the most poignant portrayals of lifelong disappointment can be found in the movie *Amadeus*. Once the German court composer, Salieri, broken of spirit and spending his last pathetic years in a mental hospital, asks God reproachfully why, inasmuch as he was a deeply religious and penitent soul, he was fated to be a second-rate musician, yet one who could appreciate first-rate music. On the other hand, his nemesis, Wolfgang Amadeus Mozart, portrayed as a tasteless, unrefined, and degraded alcoholic and womanizer, was born with the gift of effortlessly being able to write the most lyrical of music. The pathos and irony are not lost on the priest to whom Salieri is complaining, whose expressive face constantly reveals his distress, nor, of course, are they lost on the movie audience.

Severe, chronic disappointment can be the emotional fate of many persons. This can affect individuals, mar-

ried couples, mothers and fathers, parents and children, those who go to school, and those who must make a livelihood from work and careers. These and other threats and harms afflict a high percentage of persons. One's mother or father does not provide needed love, one's spouse is not what had been hoped for, one's work career is a failure, one's child never amounts to much or happens to be resentful toward one, and so forth.

In some urban communities, even in the wealthy United States, there is real danger of being killed or maimed by marauders or by stray bullets from a gang war crashing into one's home. The list of stresses for any given individual expands to cohorts or groups and differs on the basis of that group's goals, values, and experience. One's race, ethnicity, country of origin, economic circumstances, goals and values, stage of life, health, and so forth matter in this respect. There is little point in listing all such stresses. The central point remains the same. Life typically includes a wide variety of sources of threat and struggles to deal with them.

Consider too the warnings that Americans have been exposed to since the tragic attack in New York City on September 11th, 2001, which affected some directly in the form of death, loss of loved ones, and economic catastrophe. There are now national concerns over the threat of war, bombing, and viral plagues as a result of attack by hostile nations or groups to name only a few. And what should be said about the millions, perhaps billions of people all over the world in the past century and this one who have been victims of racial hatred, murder, genocide, war, terrorism, starvation, uprooting, displacement from their country of origin, and resentment toward them as immigrants.

Can readers who remain truly aware of the world in which they live believe I have overstated the stresses and struggles that people deal with in their lives? One should not be fooled by nice talk about all the good things in life into believing that to be generally at ease, happy, and sanguine about one's life is automatic or effortless. One of the ironies of life is that ailing and disabled people who still have verve for living, or married couples who remain in a long-lasting loving relationship, are sometimes belittled by people's saying they are lucky.

Let positive psychologists scoff, but it doesn't change the facts of life for most people, however sanguinely they portray themselves. One should not deny that calamity and death are always present even if one avoids thinking about them. Life is always a diverse mixture of emotions, both positive and negative, rather than a never-ending delight. Nevertheless, acknowledging the realities does not prevent people from also seeing the positive aspects of life.

To speak of what is positive in their lives as good luck is to ignore or downplay the coping processes that

must be brought to bear against the ordinary downhearted moods of life or a harsh fate. One of my hobbyhorses in this essay is that as human beings, we cannot adequately understand our emotional lives without reference to the coping process and the appraisals that influence it.

Coping and Emotion

Three commentaries are especially important to this topic, which centers on the coping process. They consist of the commentaries of Tennen and Affleck (this issue), Folkman and Moskowitz (this issue), and Campos (this issue). Without intending to denigrate the substantial substance other commentators have contributed, I note that what these three commentaries say about coping and emotion is important enough to have waited until now. I want to emphasize the importance of coping in orchestrating the balance between positive and negative emotions. I consider this one the most important message I have to offer, and I saved the commentaries that make this point strongly for last.

First, I address the Tennen and Affleck (this issue) commentary. I have the impression that I hardly need to defend what I said in my target article because Tennen and Affleck did it so ably and tersely. They might be even better qualified than I am to have written the target article in the first place. I have for a long time greatly respected their work and thoughtfulness, and I am grateful to them for providing artful and effective support. Their research exemplifies, in my view, an ideal of understanding about the virtues of ipsative–normative research designs and in-depth study of human adaptation. In case those who are now reading this have not yet read Tennen and Affleck's commentary, I quote a short excerpt from their summary for the purpose of giving it the emphasis it deserves:

The good news for positive psychology is that Lazarus's (this issue) critique and our commentary are based neither on suspicion nor on the assumption that positive psychology reflects hucksterism on the part of its proponents. The bad news is that these critiques present, we believe, a far more formidable challenge. A positive psychology that declares its independence from research in the areas of stress, coping, and adaptation; that insists on making qualitative distinctions between seemingly positive and negative human characteristics; that determines *a priori* and without attention to context those characteristics that will be studied as strengths; that follows psychology's most intractable methodological bad habits and then wears those habits as a merit badge; that distances itself from its predecessors; and that dismisses its critics as suspi-

cious or closed-minded is, to use Lazarus's terminology, a movement without legs.

Now for the Folkman and Moskowitz (this issue) commentary. Susan Folkman and I were collaborators during the last 10 years of the Berkeley Stress and Coping Project, both in research and theoretical writings. I am also familiar with the research of Folkman and Moskowitz on caregivers of partners with AIDS, with which I was greatly impressed. I begin my response to their commentary by noting a minor misunderstanding that should be corrected.

In my target article, I asked a rhetorical question rather than making an assertion about how positive emotions arise. The question I asked was "Are we really to consider as representative of positive psychology only people who live under highly favorable circumstances?" This was a preamble to the subsequent point, which was exactly the opposite of what Folkman and Moskowitz state. I wrote the following in answer to my question.

Stress and loss are inevitable features of life. To be able to transcend the harsh realities seems to me to be a better approach to what is meant by positive than what is suggested by those pushing the positive psychology movement, which has a Pollyanna quality.

In effect, my answer to the question I posed was no, we shouldn't consider positive emotions as arising from only positive life circumstances. They often come from transcending harsh realities. I am in complete agreement with these authors that the heart of this issue about positive and negative emotions is the complex causal relationship between coping and the emotions.

Folkman and Moskowitz's (this issue) basic argument has to be emphasized especially in light of the disinterest in coping displayed by those who are smitten with the positive psychology bug. I guess coping implies stress to them and, therefore, violates their stated doctrine that psychology should de-emphasize stress, struggle, and crisis. As I said earlier, the arena of stress and emotion, and much of positive psychology itself, cannot be dealt with adequately without reference to the coping process.

To make their point about positive emotions, Folkman and Moskowitz (this issue) present a brief fragment of an interview with a maternal grandmother whose daughter was diagnosed with HIV at 16 and died in the past year. She is now taking care of a 9-year-old grandson who was diagnosed as having been infected with HIV at 4 months of age. It is an awful tragedy, and she now lives with its sequelae. Yet, in the interview she talks in mostly positive affective terms. To make what I say about this understandable,

the interview fragment should be reprinted here. This is the grandmother talking:

I got the best job [delivering newspapers] in the world. I've made it grow. And I'm doing everything I want to do, because I love driving, I love meeting people. I love being able to help our elderly, because I think they've been robbed of so much. And so for me it's been seventh heaven being able to just get them their newspaper, which is a little thing. But it means a lot to them.

Unfortunately, we know nothing from this interview fragment about other interviews that have undoubtedly preceded it. What the grandmother says seems genuine, but my hunch about her immediate posture is that she is determined to present herself to the interviewer as feeling very positive about her life in spite of the awful situation she is confronting.

Everyone has an underside. The grandmother is probably understating the distress she has felt some or perhaps much of the time, but we cannot tell this from the information the authors present here. Most likely there must have been some grieving, which is usually a search for a viable meaning with which to understand what has happened. Perhaps she is still searching for meaning and trying to convince herself. It is difficult to be confident about any of this without knowing more than is available in this interview fragment.

A proper clinical interview is a transaction between interviewer and interviewee. In this grandmother's five or six brief sentences, we do not know what the interviewer actually said. I would have wanted to know whether there are times she feels downhearted or depressed. How does it go from day to day? What does she feel as her grandson shows signs of deteriorating medically? What does she expect to happen? How does she manage to remain emotionally so positive, if that is a correct description of her state of mind?

For these reasons, this interview fragment is not ideal to illustrate Folkman and Moskowitz's (this issue) point about positive emotions, negative emotions, and the role of coping. The grandmother's intrapsychic balance of positive and negative feelings is apt to shift over time and circumstances, perhaps even in the same session, and certainly over time and across interview sessions. This is why searching interview questions are necessary to understand fully what is happening. Folkman and Moskowitz probably have these data and can resolve this issue if they have not already.

An emotional pattern may say as much about coping as about affective tone, which I believe is Folkman and Moskowitz's (this issue) intended message. I remember their earlier suggestion on the basis of their AIDS caregiver data that, given the need to cope with unrelenting caregiver stress, positive feelings are apt to be less dependent on naturally occurring positive e-

vents and have more to do with a caregiver's efforts to make such events happen or to appraise neutral natural events in a positive way.

This says that coping and emotion are interdependent concepts, which should not be separated except for the purpose of identifying and describing them verbally. They must be studied together. This is a very important message, and the resolution of the causal linkages between coping and both positive and negative emotions would be a significant step toward advanced knowledge.

Now for the Campos (this issue) commentary. Joseph Campos and I have been close colleagues at Berkeley, and I have a high regard for his wisdom and acumen. He has dealt exquisitely with two themes that lie at the heart of what is wrong with positive psychology thinking. His commentary also adds a missing developmental focus to this debate, which is based in part on his influential research on a baby's way of coping with the fear of height.

Campos (this issue) presents two cogent themes about which he writes lyrically. One theme is the absolute necessity of treating positive and negative emotions together, as indissociable or, as I sometimes put it, interdependent. The second theme is that coping and emotion belong together; one cannot be separated from the other. The whole is more than the sum of its parts.

I consider his analysis of the the Albee play *Who's Afraid of Virginia Woolf* as marvelous in support of his two themes with its superb handling of the difference between the superficial and the deep. The contrast between the two couples points up the interdependence of positive and negative emotions and the central role played by coping. Things are not what they seem, he states. The scorn shown by the older couple, which seems on the surface to be irreconcilable, actually turns out to involve genuine bonding, caring, and love. The younger couple turns us off despite their civility and diffidence. The audience ultimately becomes aware that they will not be able to make it as a couple.

I have seen the movie many times and always marvel at it. As in the Folkman and Moskowitz (this issue) commentary, and Ryff's (this issue) too, positive often comes from negative. Coping, which in the older couple's relationship is a recurrent and nasty interpersonal game, actually preserves what is caring in their relationship. It is the fulcrum that, as Campos says, makes the cognitive-motivational-relational processes essential to the emotions experienced and interdependent in the emotion arousing process.

For those who might not have read his commentary, I cannot resist quoting part of it here for repetition and emphasis. In most respects he should have the last word in thinking about the emotions. Campos (this issue) writes as follows:

There is another way [in addition to coping] in which an emphasis on positive emotionality leaves out crucial aspects of human development. The core of one's personality rests in the attachment relationship of the developing child to his or her principal caregiver. Positive psychology would have us emphasize the tenderness; the smiling; the playfulness; and the pleasures of eating, drinking, and bathing as critical components of the attachment relationship. However, as Bowlby (1973) argued, the core of the attachment relationship is formed only when negative emotions, particularly fear, are generated and the caregiver then serves as a crucial and needed haven of safety, a source of protection and comfort.... Viewed this way, negative emotions are necessary for the infant and child to experience love and to build up secure knowledge that he or she can explore the world and learn from it as a result of the relief from fear and threat that the caregiver has previously provided.

Observe what happens when a child begins to walk. The child expands his or her explorations farther and farther from the parent, but without fail, the child reaches a point in his or her travels when he or she appears to have gone too far and then turns around to return to the parent. The child first "touches base" with the parent, so to speak, then initiates anew the explorations of the world.... At the very moment that the child is exerting his positive emotions of joy in exploration, he or she is rendering himself or herself vulnerable to the dread of separation from the parent. So, he or she is motivated to return to the parent for a boost of security.... [P]ositive emotions and negative emotions are indissociable in the developing world of the infant.

How Did the Worrisome Methodological Problems Come About?

Now from the sublime to the ridiculous! This seems to be the right moment to consider what happened to lead to this theoretical and research debacle I have been writing about. I close my author's response with some speculations. I believe that we need mainly to consider the incentive system under which academic researchers operate today as the main culprit.

More than a century ago, most scholars and researchers—for example, Darwin and Galton—were independently wealthy and did not make a living from research. They pursued research on their own, sometimes obtaining funding from special interest groups. Today, probably all academicians do research as part of a career, which is also a job from which they make a living. This makes them also beholden to outside funding.

Having a paying job is essential if one is not independently wealthy, so I have no quarrel with this. However, it also makes for the time pressures characteristic of academic life, which have increased alarmingly in the past decades. There is no need to detail them here

because they are well-known to most readers. The most harmful consequence is a serious lack of time to do what a research scholar must do: read; write; evaluate; plan; collect research data; and above all, think.

Career advancement increasingly depends on a type of research in which data can be collected, written up, and published rapidly. Academic research scholars must be maximally efficient, which means they cannot waste too much time on nonessentials. If they do not publish in quantity, they are in danger of being denied a job, tenure, promotion, and pay increases. The competitive standards of the most distinguished institutions have also percolated down to most universities and colleges.

Competition is by no means entirely bad. Yet in academe it has reached the stage of creating a zero-sum game, at least in the minds of faculty members. Any benefit to a colleague is then taken as a threat to one's own well-being, an outlook that must be fought by everyone in university life if we, as academics, are to retain our integrity and that of the system.

Those who might defend the standard institutional arrangement might deny the truth of this—the stated criteria for promotion in public universities, at least the ones I know about, usually emphasize the quality of a body of work in written documents and often say little about quantity. It is probably true, however, that the actual criteria are the other way around with quantity having the upper hand.

In any case, publishing impact is nationally and internationally the coin of the realm. I knew this in my first academic job in 1948. My chairperson was very clear about what was important, yet some faculty members didn't get the message, much to their regret. Most, however, are well aware of the truth—but being a whistle-blower is dangerous, and those who are successful believe they are thriving, so they do not want to rock the boat. Haste and sometimes cynicism has become the enemy of the whole system.

If one accepts my description, one conclusion is that faculty members cannot afford to spend years prior to publishing in order to polish their methodology and measures of the phenomena they want to study or to replicate their research. Granting agencies will not pay for a long-term investment in preparatory research and data collection or in replications. Nevertheless, pilot research is often as important to the field as data collection, findings, and frequent publication. I have often been unable to analyze useful data because the money to do so was not available—so the data had to be shelved and ultimately trashed, never to reach the light of day. To succeed, faculty members must gain the attention of others in the same field, which is even more difficult to do today than when I was just starting out.

Thus, in a crucial sense, institutional arrangements affect the kind of research most of us, as researchers, end up doing. It seems unfair, therefore, to blame re-

searchers for a pattern that is not easily changed. The current incentive system has evolved over a long time. Occasionally, one hears about a proposal for change, but nothing ever seems to be done. To change would require a nobility of spirit to which all but the most visionary persons would be unlikely to commit themselves.

Though it is by no means a panacea, longitudinal research, which I presented as one of the main alternatives to cross-sectional and experimental strategies, is especially expensive and often extends over a long time period, sometimes the best part of a professional life. The data collection must be carefully done and can be enormously time-consuming. There can be a mountain of it to analyze and write up when the study is finished. When researchers begin a longitudinal study, the common outlook about the proper way to measure the variables and what are considered to be appropriate questions to investigate may change in relatively few years. The fashions that dictate what seem like fruitful research problems change rapidly.

Ipsative–normative research and research with a single participant, which is a short-term longitudinal study, must depend on a very limited sample. Therefore, a number of studies are needed to permit the researcher to generalize to other samples or the population as a whole. Is it any surprise that cross-sectional research, which is easier and faster to do than any other format, remains the norm? All in all, many powerful factors pull the researcher to do something quick and dirty and publish often. Perhaps no one will notice that few, if any, research findings have been replicated or that the basis for causal inference is weak. I believe this is a corrupting arrangement, and if it is not somehow fixed, the future of our field is apt to be compromised.

In coming to the end of my author's response, I bid my farewell to the reader with two closing thoughts. First, regardless of whether commentators' responses were favorable, unfavorable, or mixed toward my target article, and assuming they dealt with the task of commenting respectfully and with integrity, I appreciate their efforts, though I have not hesitated to be critical.

Second, I am grateful for the opportunity to pursue some of my intellectual hobbyhorses, even if others might consider them idiosyncratic. I hope that some of the young people who are being led down the garden path of simplistic thought and weak research methods will read and give serious thought to what I have said. Their understanding and ability to further what we know is crucial to whether psychology, especially one that is focused on emotions and adaptation, is to make any real progress in the future or just continue to stagnate. At my stage of life, I do not need another publication, but I believe elder statespersons have a responsibility to advance their field to the extent they can.

Note

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